Afroasiatic Linguistics

Bibliographical Bulletin

BIBLIOGRAPHIC BULLETIN

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1. GENERAL STUDIES

1.1.   **Muhammad Hassan Ibrahim,** *Grammatical Gender: Its Origin and Development* (Janua Linguarum, Series Minor, 166).
The Hague: Mouton, 1973. 113 pp.
   By *ALAN S. KAYE* **(California State University, Fullerton)**

The subject matter of this book should undoubtedly be interesting reading for the general
linguist as well as for the Semitist, since Arabic is the language most talked about — a fact
given away, probably by the author's name. Ib. (a linguist at the University of Jordan) is
a well known personality in Middle-Eastern linguistics who recently taught a course in
"Introductory grammar" at the Third Middle East Linguistic Institute, summer 1975, at Cairo
University (see *LR*, 18:3, Nov. 1975- 1).

The volume consists of ten chapters and it is my intent here to bring out the highlights and
discuss some of the problems of each. It is good to see, incidentally, by way of getting
into the topic, a rebirth of interest in this area since the pioneering efforts of Benjamin
I. Wheeler almost eight decades ago.

Ch. I, Introduction (11-13) surveys the relative neglect by grammarians on problems of the
definition and function of gender in language since Jespersen. The author impresses on the
reader that a study of the gender systems (or noun classes) in the world's languages will

provide great insight for language universals, which was a goal for anthropological linguistics as far back as Boas' *Handbook of American Indian Languages* (1911).

Ch. II, Speculative theories of gender (14-23) deals with some of the older literature. Two and a half millennia ago the Sophists first noted that gender sometimes functioned in terms of concord and other times reflected a more concrete realization of nature, i.e., sex. The ancients, like most modern laymen, were of the opinion that a language would morphologically characterize an object if and only if the culture needed to differentiate between male and female versions of a specific lexeme, i.e., ethnolinguistic reasons (cf. Ib.'s examples of Latin *EQUUS* and *EQUA* quoting Robins' *A Short History of Linguistics* (1967)).

More recently 18th and 19th century philologists proclaimed that masculine gender was invented due to a description of the nature of man (such attributes as size, physical strength, brutality) and delicate qualities (passive nature, fertility, delicateness) reflected woman, hence feminine gender. Ib. supports this philosophy of grammar by examining some of the writings of Herder, Adelung, and most influentially Jacob Grimm. In 1900 Sir James Frazer rejected Grimm's theory and sought the origin of gender in men's and women's speech (cf. Sapir's writings on Yana, Mary Haas' on Koasati, and most recently Mary R. Key, *Male/Female Language* (1975) for the issues involved).

Turning to some of the standard works on Semitic, Ib. sees an influence of the IE grammatical tradition on such Orientalists as Wright and Gesenius for they also conclude that the Grimm theory was the correct one, viz., masculine reflects male attributes and feminine female. However, some Semitists looked to folkloristic interpretations as we discover when Ib. quotes the little-used book by A.J. Wensinck (*Some Aspects of Gender in Semitic Languages* (1927) that the reason most body parts are feminine in Semitic is that "they are considered as seats of magical energy" (21).

Even Brockelmann's *Grundriss* (vol. I (1908)) entertains the speculation that early Semitic society was matriarchal, and as such, PS *ʿayn, *ʾuðun, etc. had no overt feminine marking.

Ib. proceeds to survey Arabic medieval grammatical thought as it relates to gender in Arabic. Ib. is correct in seeing this tradition as a descriptivist one, i.e., a word was feminine because "they were heard from the Arabs [bedouins] as feminine" (*liʾannahā sumiʿat ʿan ilʿarab muʾannaθah*) (22). However, the Arabs too indulged in speculative hypotheses by using synonymy association (*lisān* 'tongue' would be feminine if it were used to mean 'language' by association with *luγah* 'language') or ellipsis (proper nouns are feminine because *madīnatu* 'city of', e.g., was deleted but still understood (23)). It is interesting to note that Arabic is still taught with similar explanation and most native speakers who are teachers are well versed with above-like notions. This phenomenon could easily be considered a myth about Arabic not discussed in Ferguson's classic paper ("Myths about Arabic," *Georgetown University Languages and Linguistics Monograph Series*, No. 12 (1960) nor mentioned in L. Nader's reaction to that article ("A note on attitudes and the use of language," *AL* (1962)).

Ch. III, The nature and function of gender (24-29) is involved with the way 20th-century linguists have dealt with gender as part of grammatical categories in general. A surprising omission of Whorf's "Grammatical categories" (*Lg.* 1945) catches the eye as it would have enhanced the entire chapter. It is still valuable to talk in terms of cryptotypes and phenotypes, selective and modulus categories, and part of Whorf's article (90-93 in the reprinted version in J.B. Carroll, Ed., *Language, Thought and Reality: Selected Writings of Benjamin Lee Whorf* (1956)) deals with gender as a cryptotype (covert category) in English and noun classes in Navaho similarly ('sorrow' is in the round class).

No one will argue with Ib. when he states (24, and for some unknown reason four other times throughout the work) that some languages lose gender (e.g., Persian) and there is a language universal that no language reacquires a gender system after having lost it with the possible exception of Rumanian (24,fn.1, quoting Rosetti (1965) "Remarques sur la catégorie du genre en

roumain" in *Linguistics*). A recent article "Rumanian gender" by Robert N. St. Clair (*LS* 21, Aug. 1972, 20-26) points out that there is one stem underlying both masculine and feminine adjectives and that a vowel apocope rule conveniently describes the masculine (assuming -*u* in underlying forms) and "a mixed gender redundancy rule, thereby allowing us to characterize all of these processes of gender adjunction by one and the same morphological rule" (25).

It is quite relevant to pursue the relationship between grammatical gender and linguistic typology (27ff.). Thus one can ask if there is any correlation between TYPE of language and presence of gender. Some agglutinating languages (Caucasian) have gender although most probably do not, inflectional ones (Indo-European and Afroasiatic) largely do, many poly-synthetic ones do (Amerindian), but gender is unknown in any root-isolating language (Chinese). Thus it is tempting to theorize that as languages drifted from synthetic to analytic (loss of case and mood for Arabic, etc.) they also TENDED to lose gender, although all languages known preserve sex differentiation terms, e.g., 'mother, father' being represented by two lexemes (one of Greenberg's universals).

Ib. violates the no-value-judgement criterion in terms of the lexicon and simplicity (follow-ing J. Gruber, *Functions of the Lexicon in Formal Descriptive Grammars* (1967)) (28-29): "It is more favorable for a language to use derivational means for getting new words than to make up entirely new ones...." This implies, of course, that Arabic *kalb*, *kalbah* are more "logical" than their glosses 'dog, bitch' or 'he-dog, male-dog', 'she-dog, female-dog'.

Ch. IV, On the origin of gender (30-50) is divided into two parts. Part A (31-39) on Indo-European, summarizes the views of J. Schmidt (inventor of the wave theory), Paul (*Prinzipien der Sprachgeschichte*), Brugmann, Meillet, and Lehmann. One universal emerges from the dis-cussion (37,fn.17) quoting Lehmann: "The neuter gender developed late in Indo-European, not before the time of the development of masculine and feminine gender." This goes hand in hand, as Ib. correctly notes, with Greenberg (*CTL*, vol.III, Theoretical Foundations (1966)) that the neuter is more marked than either the masculine or feminine.

Part B on Semitic gender (39-48) deserves some comment. Most Semitists would, I think, agree with Ib.'s pessimism when he maintains that Semitic linguistics is not so developed a field as Indo-European linguistics, but some might disagree that the reason for this is the Semitolo-gist's lack of knowledge in the Indo-European field (scholars like Kuryłowicz and S. Levin, the latter principally in a long book *The Indo-European and Semitic Languages* (1971)) are equally well-versed in the Indo-European field.[1]

The bulk of this section deals with Brockelmann's opinions (*Grundriss* (1908) and *Précis de linguistique sémitique* (1910)) and those of Speiser ("Studies in Semitic formatives," *JAOS* (1936)). Basically the Brockelmann view can be capitulated as follows:

1) Semitic uses different stems and not grammatical gender for differentiation (Arabic *ǰamal* 'he-camel', *nāqah* 'she-camel').

2) Arabic has no female marker for female states (*ḥāmil* 'pregnant' [*hublā* is a later for-mation], *ḥāʾiḍ* 'menstruating').

3) In Semitic certain words look masculine and are feminine (Arabic *ʾarḍ* 'earth') and vice versa (Arabic *xalīfah* 'caliph').

4) The -*t* was in Proto-Semitic a demonstrative element.

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[1] It is highly unlikely that Murtonen's contention of Proto-Arabic's not having gender (40) [*Broken Plurals* (1964)] can be seriously considered. The idea, however, that a gender system was originally (in Proto-Semitic) a class system has been mentioned by many (i.e., animate vs. inanimate).

That the *tāʾ marbūṭah* did not originally mark the feminine has recently again been taken up by Gelb (*Sequential Reconstruction of Proto-Akkadian*, 1969:34ff. and 74ff.). Gelb's point is that the -*t*- in *kalbatum*, e.g., was a consonantal glide between the gender vowel -*a*- and the case vowel -*u*-. Gelb also maintains that gender is identical with case/mood (76) "since the case system most probably was developed later than and independently of the gender system."

Speiser's article argued for an original animate-inanimate distinction because the -(*a*)*t* suffix has other (more archaic) functions than marking feminine among which are the following:

1) Abstract (Arabic *fiʿlatun* 'deed'),

2) Plural (Arabic *kafaratun* 'infidels'),

3) Nomen unitatis (Arabic *baqaratun* 'a cow'),

4) Diminutive (Arabic *ʿuyaynatun* 'small eye'),

Speiser concludes that the -*t*'s etymon can be found in Akkadian *yā-t-i* 'me' (marking the direct object). I might add that it would be cognate with the *t* in Hebrew *ʾet*, Aramaic *yat*, and Phoenician *ʾiyyat* "direct object markers" (Arabic *ʾiyyā* does not have the -*t* element). The idea does not seem to be a reasonable conclusion for Proto-Semitic unless one assumes that Arabic lost the -*t* element (Ib. reminds us as well that this was the thesis of Meinhof's *Die Sprache der Hamiten* (1912) (41, fn.39)).[2]

Ch. V, Gender assignment in native and borrowed words (51-62) probes certain problems in Indo-European languages and utilizes Malkiel's "hypercharacterization" (Italian dialects change *manu* 'hand' to *mana* with -*a*). A good deal of space is devoted to words for 'sun' and 'moon' in many languages based on Lévi-Strauss ("Le sexe des astres" (1967)). The mythological nature of certain words has been effectively explored in the chapter called "Word magic" in E. Cassirer, *Language and Myth* (1946), and I am somewhat surprised that Ib. did not consult it.

Some general rules for loanword gender assignment conclude the chapter (61-62). It is worthwhile noting that semantic considerations make 'coach' when borrowed into German fem. (*die*), but a 'football coach' is masc. only (*der*). It can be concluded that German speakers expect (in terms of the language) football coaches to be male. It would be interesting to see if any change is to be spotted as women's liberation spreads.

Ch. VI, Noun classes and gender systems (63-76) summarizes some of the major views on noun class systems of Bantu, Enindiljaugwa (Australia) and Caucasian languages. Ib. tries to demonstrate that an early class system for nouns in Semitic might have diffused to adjectives and verbs, and thus originally marked concord; only later did it come to be associated syntax-free. Speiser also states that concord antedates true gender (Arabic *ʾanta*, *ʾanti* and *ʾakalta*, *ʾakalti* (68ff.)).

There are several universals of the implicational type which would have shed considerable light on the discussion here (Greenberg, "Some universals of grammar with particular reference to the order of meaningful elements," in Greenberg, Ed., *Universals of Language* (1963)) among which are the following: If a language has gender categories in the noun, it has gender categories in the pronoun; a language never has more gender categories in the non-singular numbers than in the singular; if a language has the category of gender it always has the category of number.

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[2]R. Hetzron informs me that Akkadian *yāti*, dative *yāši*, should be viewed in the light of the corresponding pronouns in Agaw, viz., *yət* and *yəš*. This suggests a Proto-Afroasiatic accusative in *-*t* and dative in *-*š*, which would be cognate to Hebrew directional -*h* (Cf. Bertin, "Notes on the Assyrian and Akkadian pronouns," *JRAS* (1885) 17.65-88). This is a good example of what Gelb (*Morphology of Akkadian*, 1952:3) called an "ideal reconstruction."

A discussion of SOUS-GENRES (subgender) [Hjelmslev] follows (70ff.) with particular references to Slavic, Algonquian, Caucasian, and Austronesian making the point that (76) "it is immaterial whether a group of nouns is called a gender or a class."

Ch. VII, The evolution of gender (77-90) is devoted to a followup of hypo- and hypercharacterization (Malkiel) with considerable attention paid to the concept that languages become less marked through time, an idea which has received considerable attention by P. Kiparsky (e.g., his MIT unpublished Ph.D. dissertation "Phonological Change").

One of the universals brought to prominence by Ib. states (79) that "a language is more sensitive to gender irregularities in inanimate than in animate nouns." Arabic illustrates this for there are overtly-marked -at nouns which refer exclusively to masc. animates: *ṭalḥatun* 'P.N.', *katabatun* 'clerks', etc.

The remainder of the chapter deals with various linguistic pressures which may have had an influence on gender switch (based on Malkiel): lexical polarization (semantic opposites), lexical serialization (close-knit semantic sets), synonymy (an excellent example cited by Ib. (81) is Middle Arabic *saṭūr* 'big knife' (f.) pairing *sikkīn* 'knife' (f.), which subsequently became *saṭūratun*,[3] and finally, accidental change via misinterpretation of overt markers, e.g., Spanish *el problema* 'problem' (Greek loanwords are masculine) being treated, due to its -a, as feminine (in many varieties of Barrio Spanish, e.g.), or Latin PANTEX 'belly' (m.) > Spanish *panza* (f.) (it should be noted that there is too little evidence to show that Latin PANTEX, pl. PANTICĒS were consistently masculine for they both are very rarely attested).

Ib. builds a case around the Classical Arabic dual form *ᶜarūsān* (83 and fn.21) 'bride and groom' stating that this is the only dual noun in Arabic which is derived from a feminine base (viz., *ᶜarūs* 'bride, bridegroom'), "but designates two members of different genders. That is *ᶜarūsān* seems to be the only noun in which predominance is attributed to the feminine instead of the masculine." The fact of the matter is that *ᶜarūs* could refer to either gender so that it should not be considered a feminine base at all much in the same way that *walad* can mean 'boy' or 'girl', or that in English one can pronominalize a baby by he, she or it depending on sociolinguistic attitudes and occasionally depending on nothing at all.[4]

Checking the Lane Lexicon (Book I, Part 5, 1874, 1999) we see that *ᶜirs* meant 'wife' and 'husband' pl. *ᵃaᶜrās*, and the d. *ᶜirsān* "is sometimes applied to the male and the female, or husband and wife." Speakers of Old Arabic dialects apparently felt the need to differentiate and thus formulated *ᶜarīs* 'bridegroom' (Lane says that it is vulgar) and *ᶜarūsah* 'bride' (he also calls this vulgar). The root *ᶜrs* had originally only to do with 'marriage' or 'coitus'. Thus *ᶜarūs* originally meant 'marriage partner' or 'sex partner'.

Ch. VIII, Gender and thought (91-96) refutes the older theory which connects gender with certain processes of thought. Ib. rather subscribes to the idea that gender is an accident of linguistic prehistory and "may have a role to play in certain aspects of culture, such as myth and literature (92)."

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[3] Ib. rightly mentions other cases in Semitic, e.g., Arabic *qaws* 'bow' and Akkadian *qaštu*, Hebrew *ᵃereṣ* 'earth' and Akkadian *erṣitu*, or Hebrew *nefeš* and Akkadian *napištu* 'soul'.

[4] Before the birth of our daughter, both my wife and myself pronominalized the baby as "it," exclusively. After the baby's birth (say, the first couple of weeks) "it" varied freely with "she." Thereafter "she" became standard and regularized. Hospital employees, by the way, refer to all newborns as "he," and so does all of the professional literature about infants.

Various linguists are cited who have written about the relationship between gender and mythology and poetry. (Again I reiterate that a reference to Cassirer would have been advisable). Brugmann (93) states that for Indo-European personification of inanimate objects "the sex is usually determined by the grammatical gender." Sommerfelt reports an exception to the above by citing poetical Norwegian's use of 'moon' as feminine although it is masculine in gender. Jakobson in writing about gender in Russian agrees with Brugmann and offers some unique thoughts on Russian (quoted in full in Ervin [*Word* (1962), 260-261, fn.20]): "Russians personify Monday, Tuesday, and Thursday as masculine and Wednesday, Friday, and Saturday as feminine, without realizing that this distribution was due to the masculine gender of the first three...."

Jespersen is mentioned as the discussion (94) turns to metaphorical gender in English (boats are regarded as feminine because of 'she' pronominal reference), and the chapter ends with the parallels in Arabic (95-96) such as ʾummu lbayt 'housewife' (lit. 'mother of the house'; cf. Hausa *uwargidā* < *uwā* 'mother' + *gidā* 'house' = '(first) wife'), ʾummu lqurā 'mother of the towns' = Mecca, etc.

Chs. IX and X, Gender in grammar and concluding remarks (97-104) can be handled together since their subject matters are in essence the same. Ib. states (101) that Transformational-Generative grammar along the lines of Chomsky's *Aspects* (1965) "seems well-equipped to handle, most, *if not all* [italics mine] of the problems related to gender" reaffirming this by citing Gruber's use of the disjunctive ordering to handle the lexical representations of ram, lamb, ewe, and sheep (100). Ib. (103) refutes Fodor's conclusion that "the category [of gender] cannot be treated in generalized terms even within one language family," yet I think both positions are untenable at present since linguistic theory and methodology cannot account for the few cases of nouns which do not fit the general principles (e.g., German *Kind, Fräulein* being neuter). The question as to why certain languages have gender-class systems and others do not is like asking why certain languages have dental stops and others do not, i.e., can not now receive definitive treatment.

There are two areas which need to be covered that Ib. might have pursued with fruitful results. Firstly is that gender assignments may have arisen to differentiate morphemes which became homophonous or were on the way to becoming so, e.g., Italian *banca* (feminine) 'bank (for money)' and masculine 'bench, counter'.[5] Secondly gender differences express different social attitudes on the part of the speaker and hearer. By this consider the following: "She runs well" when speaking about a car expresses a certain affection for or sentiment about the automobile, however, "I want to junk it" connotes a lack of affection (even anger and contempt).[6] A recent study (Dell R. Marcoux, "Deviation in English gender," *American Speech* 48 (1-2) (1973, published 1975) 98-107) emphasizes free variation — a point which is valuable in accounting for certain problems in Semitic gender, e.g., why certain nouns belong to both genders (the usual answer here is different dialects). As Marcoux concludes (107): "The native English speakers in this survey did not consistently choose pronouns according to the natural gender of the antecedent noun or according to fixed grammatical gender of nouns denoting ships and countries."

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[5]Cf. G. Bonfante, "Semantics, language" and S. Ervin, "The connotations of gender," which uses Bonfante's study as well as listing another two dozen plus examples (both studies are mentioned by Ib. in the bibliography). 'Bank' is sometimes also *banco* (masc.) in Italian, in the names of old and prestigious banks.

[6]"*I want to junk her" would certainly be ironic, but may occur. The sentiment can be illustrated by the following: "I've worked on the damn thing for an hour, but *she* (indicating sentiment, fondness, attachment, etc.) still won't start." The meaning of 'resentful sentiment' is also possible.

On the whole Ib. did justice to the topic. However looking into this topic in the future
will necessitate the fruitions of sociolinguistics. By this is meant that no longer does it
suffice to see language as a conveyor of information (and language categories too) but also
as a means for social interaction.[7] Thus gender may turn out to have its origin and present-
day function in marking human relations, i.e., people's images of themselves and of other
people in intimate and non-intimate situations alike.[8]

2. SEMITICS

2.1.   Margaret K. Omar, *The Acquisition of Egyptian Arabic as A Native Language*. Janua Linguarum Series Practica, 160.
The Hague: Mouton, 1973 .
By *PETER F. ABBOUD*   (University of Texas, Austin)

This is the author's doctoral dissertation presented at Georgetown University. It purports
to study the acquisition by children of the phonology, vocabulary, and certain selected syn-
tactic and morphological features of Egyptian Arabic. It is based on field work conducted
during four months in a small village in Central Egypt on 37 children ranging in age from 6
months to 15 years. The data consist of recordings of free and induced dialogues and conver-
sations, and comprehension, imitation, and grammar elicitation tests, where appropriate. The
presentation includes not only the data and the results, frequently given in diagrams and
tables, but also a detailed discussion of the theory of child acquisition and comparison with
the findings of similar studies in other languages.

The study consists of 7 major sections and an extensive bibliography. The first two sections
entitled " Introduction (1-12), and Basic Characteristics of the Physical and Family En-
vironment (13-22)," are introductory in nature, and set the stage for the study. They include
in addition to the usual preambles the vital statistics of the children in the study, an in-
teresting and insightful discussion of the social structure of the community and the function
of language within it, and a brief sketch of the language itself. The third section "The
Development of Phonology" (23-73), which forms the major part of the work, deals with the
acquisition of the phonology. It discusses the phonological system in adult language (about
which more later), the babbling stage, the stages of acquisition, of which the author sets up
somewhat arbitrarily three stages with intermediate stages between the second and the third,
the order of acquisition of phonemes and phonemic sequences by age of acquisition (by one
year five months, two years etc.), and a discussion of sounds obtained in imitation tests.
The fourth section "Early Communication and Initial Vocabulary" (74-92) discusses the results
of a comprehension test and a list of early vocabulary. These are difficult areas to study

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[7]Or put differently, language is not only a tool for communication but also a vehicle by
which humans react to, adjust to, cooperate with and live side by side with other humans.

[8]The most important article to come out in recent years on a semantic-sociolinguistic ap-
proach to this entire problem is T.F. Mitchell's, "Aspects of gender revisted, with special
reference to Sindhi and Cairene Arabic," *Archivum Linguisticum* (New Series) (1973) 4.27-50.
Mitchell proposes to abandon distinctions of grammatical gender in Cairene. He concludes
(43): "What is essential is a functional perspective comprehensive enough to support a
general theory of language as part of social life, yet specific enough to orient empirical
research." Mitchell's paper has been rewritten for his *Principles of Firthian Linguistics*
(137-153) (Longman Linguistics Library, 1975).

and the results are tentative, especially in view of the conditions and limitations of the study and of many extra-linguistic imponderables such as culture, intelligence etc. which play a crucial role. A third subsection deals with Baby Talk, giving an inventory of items and their linguistic and cultural features. It is interesting to note the widespread use of this phenomenon and of the items themselves not only throughout Egypt but in many (probably all) parts of the Arab World. The fifth "The Development of Syntax" (93-135) and sixth "The Development of Morphology" (136-186) sections are similarly organized. The first subsection in each deals with early stages of syntactic and morphological development respectively, the former in terms of one-word then multi-word constructions and the latter in terms of noun and adjective inflections. Then follow one or more subsections entitled "The Acquisition of X", each with further subsections dealing with rules for X in adult language, a description of the test devised to obtain data for X, and finally stages for child acquisition of X. Two syntactic features, the negative and the interrogative, and two morphological features, inflectional affixes for noun plurals and inflectional affixes and agreement for adjectives, are selected for study. At the end of each section is a useful summary recapitulating the major findings of the section. A final section entitled "Conclusions" (187-196) gives an overall review of the findings and conclusions, discusses the degree of support the study lends to various theories dealing with primary language acquisition, and presents a well-thought out list of suggestions that future work on child acquisition of Arabic should consider.

Having briefly summarized the work in order to give the reader a general idea of its content, coverage, and organization, we are now in a position to give some general comments and evaluation.

1. One of the important objectives of this study is the comparison of the findings with those of similar studies on other languages. This is readily seen not only in the extensive notes and comments throughout the work, but also in the very choice of the features selected for study: it is obvious from various remarks in the text that these selected features are those which have been most often studied in other works. This is of course legitimate and justifiable. It has to be remembered, however, that the results and conclusions are valid only in as much as the feature chosen plays an equally significant role in the cultures where the various languages compared are spoken; as the author herself points out quoting Levi-Strauss approvingly: language is the result of culture, a part of culture, and a condition of culture. From this point of view, then, it looks as if the selection of the grammatical category "number" for study, especially the acquisition of the dual and numerals is somewhat inappropriate, since contrary to the European languages and cultures upon which most available studies are based, the culture in question is not "statistics" oriented, avoids precise reckoning, and is patently vague as far as numbering, color, sizes, etc. are concerned (observe the purposeful shying away in Arab culture as a whole of giving "vital statistics" such as age, size of family, income, and the general tendency to give vague estimates of time, distances, etc.).[1] Can young children, therefore, in this culture be expected to give responses such as "11 dogs" or "2 roses/flowers" etc. at an early age? It is quite clear that the reason why "colors and numbers are learned as late as age 6, which is later than that found for children in other studies" (188) is definitely cultural and the statement is meaningful only in a comparative cultural context. The same argument can be given for another area selected for contrastive study: the interrogative. The author in a keen observation reports that in this culture children don't ask questions (88) but are expected to listen (20). This is in contradistinction to some European cultures or even to middle-class urban Arab culture. The situation here is somewhat mitigated by the fact that "the function of the interrogative itself is simple in this language" (135). What if it weren't? Would contrastive conclusions as to relative age of acquisition of this feature have been justified? These remarks should not be

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[1]Needless to say that no value judgement at all is intended by these remarks. It might be added that this culture has clearly defined notions as to family relationships etc. in comparison to which other cultures can be considered "imprecise."

interpreted to mean that investigation of such features should not be undertaken. What is meant here is to caution against quick jumping to conclusions as to the validity of observations based on contrasts with other studies, observations which abound in language acquisition studies with their glib talk of "universals" and the like. Inherent in these remarks is also perhaps the suggestion that one would have much rather seen a study of features that are of intrinsic interest in Arabic such as (and here one obviously reveals one's bias as an Arabist) the acquisition of hollow verbs, ablaut, roots, epenthesis, male-female differentiations in emphasis, etc.

Another criticism in a similar vein is the continual contrasting, in this as in other studies, of child language with adult language, as if the latter were necessarily the model after which children pattern their verbal behavior. This is especially the case in view of the astute observation by the author that, "Most teaching of the younger children, linguistic or social, is done by older children... Children are constantly surrounded by speech, most of the time that of older children... There is usually little concerted effort put forth in training children by adults" (17). To the statement by the author that there are sentences "which are hard to characterize in terms of adult grammar" (96), with which one fully agrees, one can add that it is equally hard to identify and characterize in terms of adult grammar parts of speech, or "phonemes," or clusters. For example, why should [hub] translated 'pick me up' (84) and [dizə] translated 'female breast' [which] must mean 'I want to eat' (85) be considered verbs or verb phrases? What is a "long vowel" in the speech of very young children (38) and where does a short vowel end and a long one begin? One can further inquire what the significance of relative frequency of occurrence of phonemes is, seeing that the one adopted here (31-33) is based on a "running" text of adult speech; isn't the incidence of occurrence of even the least frequent (0.2%) of consonants (/z̧/) (ignoring the /q/) and the least frequent (also 0.2%) of vowels (/ō/), extremely high in an environment where one of the children is called /z̧āfir/, and where /bōbi/, the baby talk word for dog, is heard?

2. In view of the pioneering nature of the work in child acquisition of Arabic, of the conditions under which the study was conducted, such as the relatively short period of observation, the inability to follow an individual child over an extended period, the gaps in the ages of the children, and of the necessity of limiting the number of features studied, it comes hardly as a surprise that much of the evidence presented tends to be inconclusive and many of the conclusions are tentative. The author of course realizes this, and throughout the study is careful to point it out and to couch her statements in careful terms; to her credit, she shuns dogmatic utterances and is unwilling to go beyond the evidence. She shows extensive knowledge of the literature on child acquisition that appeared through the period of the writing of the dissertation and frequently quotes from and refers to it. One regrets she was not as demanding of these sources as she was of herself, and that she tends to be less critical of their findings than of hers. One would have wished for a careful evaluation and for a more critical use of these references and sources. One would also have wished that the experience and insight gained by the author in working with a language like Arabic which has very interesting sociolinguistic features would have resulted in a critical appraisal and refinement of approach and methodology of A *Field Manual for the Cross-Cultural Study of the Acquisition of Communication Competence*, on which this study is based (e.g. in the notion of tests for grammatical "correctness" which seem wholly inappropriate in Arabic where correctness is a function of schooling at a later stage). (Maybe, this is forthcoming.)

3. Though in a review of a book on child acquisition of language one might not be expected to comment extensively on descriptive linguistic statements, yet in view of the fact that this study will be followed (hopefully) by others, and that they have intrinsic interest, such comments are warranted. We necessarily have to be highly selective and will concentrate on the phonology; but in passing, a couple of questions. Why are /šwayya/ 'little bit' and /ratl/ 'a pound' adjectives (10)? Are there only three productive verbal root patterns in this dialect (11): what about the "passive" forms (e.g. /yitḍarab/) (144) and forms VII and X, or are these not productive? Are the 3 m.s. and 3 f.s. independent pronouns (11 ff.), /maṣriyīn/ (146), and /ᶜarabiya/ (183) with one glide only, or is this some sort of typographical (albeit consistant) error?

(1) The section on consonants is straightforward, but a couple of comments need to be made.
First, the sound /q/ is very rare and basically involves the occurrence of one vocabulary
item. Its inclusion in the phonemic inventory has necessitated many redundant distributional
statements and has obscured the distinction between genuine distributional gaps and analysis-
imposed ones. Note also the treatment of the glottal stop /ʔ/: "... the phoneme is late in
being articulated in medial and final position... it was occasionally missed by some children
at ages 5, 6, and 7" (53). In this dialect the occurrence of /ʔ/ in these positions must be
very rare (I was able to find a couple of items in the corpus; one, /masāʔil/ 'issues' is
hardly a village word). Secondly, one must take exception to a couple of statements: "Every
other consonant phoneme has an allophonic, emphasized counterpart in certain environments,
except /x/, /ɣ/, /ḥ/, /ʕ/, and /q/" (26). Surely the situation could not be too different
from that found in Lehn's article quoted on the same page in footnote 11. Is the /x/ of
/xāli/ (141) the same phonetically as that of /xallaṣ/ (129), for example? Another such state-
ment is: "All consonants except /q/ and /h/ may be doubled" (27). Disregarding the case of
/q/ (the last two points illustrate what was said earlier about the redundancy in the state-
ments concerning /q/), one has to point out that at least one expression which is used all
over Egypt, /rabbina yisahhil/ 'may God make matters easy', must occur in this dialect. There
are many other such words with /-hh-/.

(2) The analysis of the vowels is still more objectionable. First, is there really a high –
mid short vowel phonemic distinction in this dialect (28)? Where? What is meant by "...
/o/ is the least stable" (54). Secondly, a phonemic distinction between low front /æ/ and
low central /a/ vowels is made (28), based on a couple of "minimal pairs" (this suggestion is
traced in footnote 16 to T.F. Mitchell who based his argument on more substantial evidence).
The question is how valid is this distinction and how much is really gained by positing it.
(a) There can be no doubt but that in a very large number of cases these two vowels are in
complementary distribution. (b) That these vowels form one and not two "phonemes" is clearly
shown in many instances by the "slips" in transcription; the same word is rendered sometimes
with the one and sometimes with the other (e.g. /fægīr/ 'poor' (170), /fagīr/, /fagīra/ (174).
(c) There is an in-built asymmetry in the proposed system in that a distinction is made
between the low vowel and its emphatic counterpart, while no such distinction is made for the
other vowels. The statement that "There is no major allophonic variation in the realization
of the long vowels" (28), is obviously wrong, since in the neighborhood of emphatic conso-
nants *all* vowels have emphatic "allophones". (d) The vowel /a/ is ill-defined. It is said
to occur "before and after ... /h/, /ʔ/ ..." (29). The study itself has many counterexamples:
/hælīm/ (86), /bitæʔti/ (107), /hælla/ (131) etc. Further, in the example /ṭarabēza/ (27),
we are to understand that the final vowel is central and hence presumably that /z/ is pro-
nounced emphatic. Is this to be the interpretation in /da/ (116), /ma ... š/ (119 ff.),
/kætab/ (119) etc.? (e) Finally, the analysis produces a big morphophonemic alternation
problem: observe /madrasa ~ madrasāt/ (146), /talāta ~ tælat/ (149) etc. There seems to be
little justification, either theoretical or practical, for the obviously costly step of setting
up this low-central vowel distinction (the very small number of "minimal pairs" because of
which the distinction is made are quite peripheral to the language and the study), and the
study stands to gain little from it.

Just in case the reader should mistakenly get a negative impression about this work from these
remarks, it must hurriedly be added, if it is not already clear, that we have here a well-
researched, well-done job with clearly defined and well-executed objectives. The author has
a very good knowledge (and near-native control) of Egyptian Arabic and an appreciation of
Egyptian culture that few foreigners ever achieve. This is evidenced in this work in the
ability of the author to conduct her field work with children in a totally monolingual en-
vironment, in the correct interpretation of almost all the Arabic examples (almost all because
after checking the examples, this reviewer finds he disagrees with the interpretation of only
one), and in the insightful social and cultural comments that are made. The remarks above
were made in the spirit of stimulating discussion in a field which badly needs discussion and
prodding the author to pursue her research in this area and other researchers to proceed with

what necessarily is only a beginning, but a very good one at that. The shortcomings of the work should be viewed in the overall perspective of its being a doctoral dissertation in a relatively young field, where they are neither uncommon nor unusual. In conducting what in a small village must have been a very difficult job for a woman and a foreigner, and producing this pioneering work, the author has put us all in her debt.

2.2. M.J. Chayen, *The Phonetics of Modern Hebrew* **(Janua Linguarum, Series Practica, 162) The Hague: Mouton, 1973. 49 pp.**
   By *ALAN S. KAYE*   **(California State University, Fullerton)**

This is the most recent linguistic work dealing with EIH (Educated Israeli Hebrew), and it originally formed a part of the author's Ph.D. dissertation (*Investigation of the Phonology of Modern Hebrew*) submitted to the University of London (Department of Phonetics and Linguistics) in 1969. Part II of the work, a generative phonology of EIH, will be published soon (the preface (5) states that it is still undergoing computer verification). I heard some of the results of the research expected in Part II when I attended the ILA 1970 meeting in New York (Chayen's paper was entitled "A generative phonology of Modern Hebrew"), and the phonology was accomplished according to the principles set down in *SPE* (Chomsky and Halle 1968).

Chayen was a student in pre-WW II London and came in contact with Chomsky and Halle at MIT in 1963-64 and 1968-69. This explains the flavor of the monograph written in both the styles of Daniel Jones and the later generative school (cf. 32, fn.4, e.g.). The investigation is based on tapes of informal discussions of 29 native speakers of EIH (and confirmed, so we are told, by tapes of over 100 additional persons). The Appendix (38-45) consists of extracts of the recordings in a broad phonetic transcription (with partial intonation) of 10 native or near-native speakers with translations thereof. The author acknowledges the assistance of Chaim (sic.!) Blanc (it is always spelled Haim in Blanc's publications), Chaim Rabin, and Sholomo (sic.!) (now always Shelomo but in older writings Shlomo) Morag.

The introduction (11-12) discusses the rise and development of Modern Hebrew. Native-speaker fluency of Israeli Hebrew goes back, at most, half a century (Blanc, "Israeli Hebrew Texts," in *Polotsky Festschrift*, 1964:132, fn.3). Thus the author makes a good choice in claiming that all those who immigrated to Israel before 1955 have a sufficient knowledge to be classified as native speakers (11). However one must be extremely careful not to confuse Ashkenazoid Israeli Heb. with Arabicized Israeli Heb. (Blanc 1964:135), and it may be that the other intermediate major varieties exist as well (Blanc 1964:135). It must be kept in mind also that there are quite a few who have resided in Israel since before 1955 who can not be regarded as native speakers. The reason for this is that they live in a more or less secluded non-Hebrew language environment, e.g., Yiddish, Persian, Arabic, etc.

Ch. 1, The text (13) details the 29 informants used and merges their speech into one homogeneous EIH. Fifteen were born in Israel and fourteen were born in countries ranging in diversity of native languages from Hungarian to Persian. Insofar as the term colloquial is concerned a valid distinction exists between deliberate and rapid varieties (Blanc 1964:135 and fn.8).

Ch. 2, The vowels of Hebrew (14-20) deals with vowels and diphthongs. The basis of the classification comes from M. Halle's lectures at MIT. There are 5 degrees of constriction: (1) greatest constriction, e.g., stops, (2) turbulence producing, e.g., fricatives, (3) semivowel and liquid producing, (4) close vowel producing, e.g., [i], (5) fully open tract. Vowels belong to either 4 or 5.

The author analyses EIH as having 5 vocalic segments (*i,e,a,o,u*) and *ə* (he calls the latter an

"unstressed transition vowel"). Morag (14, fn.3) claims *a* as well citing the minimal pair *nataGti* (sic.) 'I planted' vs. *natati* 'I gave'. Chayen states "that this seems to be another example of the influence of literacy on phonetic judgement." Length is insignificant; thus Rabin's suggestion (15, fn.5) that [ka:n] <kʾn> 'here' contrasts with [kan] <qn> 'nest of' was erroneous. This is based on Chayen's informants, however. In formal EIH a length distinction of this type may be heard (Blanc 1964: 135, fn.7), and this may be regarded as a spelling pronunciation or a hypercorrection. Since a spelling pronunciation and pseudo-corrections are an actual part of the language, a conclusion can be drawn that more informants are needed before the grammar of EIH can be formulated.

Let us recapitulate the allophonics of the 5 vowels in capsule form:

> /i/   with allophones [i] (final position and open stressed syllable) and
>   [I] elsewhere[1];
>
> /e/   with allophones [e] and [ɛ] with distribution similar to that given above
>   for /i/ except [e] occurs only occasionally in final position, e.g., *šne,*
>   *šte* 'two'. The author solves a problem (*tsere* vs. *segol*) familiar to any
>   teacher of EIH, viz., [bɛn] 'son', 'son of' (construct) and 'between' are
>   all homophonous (or if a distinction is made with the latter it is [bɛjn])[2];
>
> /a/   with an allophone [a] and a possible allophone [ɑ] (lower back unrounded)
>   before ʿ and elsewhere too, e.g., [jerušalɑim] 'Jerusalem'. The author
>   claims (17, fn.11) that a tape recording of Yitshak Rabin, born in Jerusa-
>   lem, shows free variation between [a] and [ɑ];
>
> /o/   with allophonic [o] in all cases;
>
> /u/   with allophonic [u] in all cases.

A considerable amount of time is then spent discussing *ə*. Of importance to note is the fact that in hesitation *ve-* 'and' is normal over *və-*, usually with very noticeable length. There is also a good deal of free variation in this morpheme as *ve-* ~ *və-* (18 and also Blanc, fn.15), and my impression of the allomorph *u-* (e.g., *ušmone* 'and eight') is that it is bookish (discussed in 18, fn.14)[3].

The remainder of the chapter deals with diphthongs (19-20). The conclusion presented is that they consist of vowel plus jod—not monophonemically, and the author uses morpheme structure conditions to prove the point, e.g., *ojev,* pl. *ojvim* 'enemy'.

The reader deserves to be told more about a "diphthong formed with bilabial velar glide" (20, 2.10.2) than some speakers pronounce *ruwax* for *ruax* 'wind' ([ṙuwax]sic.). From my experience with native speakers there is a neutralization of *uwa* and *ua* in this environment, and thus

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[1]There appears to be some free variation, e.g., *šIvIm* ~ *šivim* 'seventy' (personal observation based on the speech of Binyamin Padeh, a native of Ramat-Gan, my informant for a course in EIH at the Univ. of Colorado, Boulder in 1969). The author states (16): "Nevertheless the degree of laxity varies with the speaker, many having a marked tendency towards tenseness."

[2]The discussion should read minimal pair — not minimum pair (16, fn.10).

[3]There are considerable morphophonemic rules for the deletion of schwa. The author's generative phonology (forthcoming) discusses this and *a*-deletion (19), e.g., *katav* 'he wrote', but *katvu* 'they wrote'.

there is no basis for transcribing *ħuwax* rather than *ħuax* (cf. the oral rendition of Classical Arabic form II, passive perfect, 3rd m. sg. *quwwima* and form III *qūwima*, i.e., they are homophonous, although there can be "forced" differentiation).

Ch. 3, Consonants (21-29) follows the analysis of Blanc (1964:136), except that Blanc includes the borrowed consonants and Chayen does not (21-22, fn.1). Aspirated plosives occur as allophones of the usual unaspirated variety in emphatic situations (22): [masp^hik] 'enough', [k^hen] 'yes' (this point is often overlooked in teaching). The voiced - voiceless opposition is rejected for the fortis-lenis contrast (*todati* 'my thanks' vs. *dodati* 'my aunt').[4] The stops are *p, b, t, d, k, g*.

The fricatives include *ḃ, v, s, z, x, ħ, š*. The stop-spirant alternation deserves some mention. *ḃ* and *v* alternate with their corresponding stops in other than initial position, e.g., *birex* 'he blessed' but *ləvarex* 'to bless'.[5] *ħ* has two allophones in free variation, [ɣ] and [R][6] (Arabicized [r̃]) and *x* may sometimes be [ħ] (Arabicized). It is curious (25, fn.12) that speakers from Arab countries have the pharyngeal if they moved to Israel only after completing primary school. Some *k*'s alternate with *x* (earlier *k*); others do not (earlier *q*). This fact plus the [ħ] (used by radio personnel) mentioned above would make a description difficult (we must wait for the generative phonology to appear to see how Chayen handles the situation). In classical phonemic terms [x] can be a phoneme by itself and also an allophone of /k/. Thus it would seem to be impossible to predict, given an infinitive like *ləxapêr* whether the perf. is *kipêr* or **xipêr*.

The glottal stop and fricative (< **ʾ, *h,* respectively) vary under unclear circumstances from individual to individual. This is also complicated because *ʾ* has another source as well, viz., **ʿ* (still present in Arabicized Heb.). From reading the discussion I have the impression of a totally inconsistent free variation (23). A recent paper by Holly Samiloff-Zelasco entitled "An acoustic-perceptual study of modern Hebrew 'alef' and 'ayin'" (presented at the 1974 North-American Conference on Semitic Linguistics, The Ohio State Univ., Columbus) argues that the determining factors in the retention of [ʾ] are "speed and style of speech." Thus there is usually no distinction between [karā] 'he read' and [karā] ~ [karʾā] 'she read'[7]. The author does explicitly state (25) that /h/ ~ [ʾ] or [φ].

Blanc (1964:136) was unsure about the status of /ts/ or /c/ calling for further investigation. Chayen argues in favor (25, fn.13) of separate phonemes but adduces no evidence anywhere in the book to support his suggestion. Haiim B. Rosén (A *Textbook of Israeli Hebrew* (1962:2-3)) analyzes both as members of the same phoneme and transcribes both as /ts/. Yehiel Hayon's recent textbook (*Modern Hebrew* (1970)) prefers a similar analysis but writes this affricate as /c/ (see "Phonetic symbols," vol. I, x). The difference in implication between /ts/ and /c/ parallels a hotly-contested problem in English phonology with /č/ or /tš/ and /ǰ/ or /dž/, which now has quite a sizable literature devoted exclusively to it (see W.F. Twaddell, "/č/?", *American Speech* 47:3-4, Fall-Winter 1972, published 1975:221-32). There is no definitive solution for the Hebrew problem anymore than there is for the English one, although the author does suggest (26) "an ambiguous pair [suggested by Blanc]": *huca* 'he was thrown out," and *hutsa* 'she was flown'. Both these words are homophonous.

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[4]In fact the author states that *b*'s, for example, scarcely have a trace of voice (22).

[5]Or as the author states in a stylistically awkward English: "Otherwise initially, these fricatives alternate with their cognate bilabial plosives (24)." *berex* is Classical.

[6]The author (24, fn.9 and *passim*) calls these velar. These are uvular (so also Blanc (1964: 138)).

[7]Blanc (1964:137, fn.12) mentions that this matter is complex and states that a syllable boundary may be audible as [ʾ], e.g., [kni(ʾ)a] 'surrender' vs. [kniya] 'purchase'.

In the remainder of the chapter a discussion ensues concerning /t/ followed by /š/ sequences vs. loanwords with /č/ (he says that a distinction should be made (27)), nasals (including a borrowed /ŋ/, e.g., /baŋk/ 'bank', although /ŋ/ does occur in native words, e.g., /maŋgina/ 'tune', cf. /hangana/ 'intonation'), borrowed /ž/, the lack of syllabicity in nasals with the exception of Arabic proper names with Ibn[8], the lateral /l/ (never 'dark'), and the semi-vowel jod (29, fn.18 quoting Blanc /idiot/ 'news' contra /idjot/ 'idiot').

Ch. 4, Stress (30-33) is very brief for this complex subject. The standard minimal pairs for stress are given (32), e.g., *kắma* 'she rose' contra *kamấ* 'she rises', *íma* 'Mother' contra *imấ* 'her mother', etc. There are some informal rules for stress given (33), and most of them are just listings of certain suffixes that take stress (4.5.2) or unclear statements such as: "In such cases where stress moves to the suffix, the vowel previously accented will lose its stress." There do not appear to be any consistent rules for stress in place names (31) and there is some degree of free variation, even with the same informant, e.g., *ắšklon* and *ašklốn* 'Ashkelon'.[9] We shall have to wait for the generative phonology to see how the entire matter is treated.

Ch. 5, Intonation (34-37) is, as the preceding chapter, very short. Falling tones (the better designation would be pitch), according to Chayen, occur in declarative sentences, 'wh'—questions, and commands and exclamations. A rising pitch occurs in yes/no questions, and at the end of subordinate clauses (36), however, it is not very clear what a subordinate clause may be. Falling-rising and rising-falling-rising intonations may occur as well (36). Blanc's texts (1964) are marked for high pitch. A detailed study of Hebrew intonation is badly needed for there is no exhaustive study available.

The book has whet my appetite for the author's generative phonology. It is the only monographic treatment of the subject of which I am aware and it is recent, yet I feel that a judgement of it on my part, without having read the generative phonology, would be premature. Outside of many stylistic infelicities and over a hundred misprints largely in the sphere of faulty diacritics and incorrect spellings in the references[10], the work should reawaken the interests of specialists in Semitic to get back to the part of language about which we know more than anything else, i.e., its sounds.[11] On the whole, however, the present work is more reliable than the phonetics chapter (1962:1-11) in Rosén's textbook.[12]

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[8]Blanc's texts have many examples of syllabic nasals (1964:139ff.).

[9]The so-called "heavy suffixes" *-tem* and *-ten*, 2nd m. and f. pl., respectively, are reported to be unstressed (30, fn.2). This is contradicted by Binyamin Padeh (see fn.1) who regularly stresses the ultimate syllable in a form like *ktavtém*. A stress such as *ktắvtem* would result by the regularization of the paradigm, e.g., *katắvti*, etc. Very normal, however, is the form *katắvtem* (not *ktắvtem*), i.e., there is no elision of the first vowel in the perf. paradigm. The simplified paradigm is gaining (Cf. W. Weinberg, "Biblical grammar/ Israeli grammar: accepted and unacceptable changes," *Hebrew Abstracts* XV (1974:37). Many Israelis insist, however, on the Biblical stress pattern.

Stress examples are perhaps most commonly heard in individual or place names, e.g., *rišon* 'Rishon le-Tsion' vs. *rišốn* 'first' or *xáim* 'Chaim or Haim' vs. *xaím* 'life' or 'they live'. Many other examples are given in this chapter.

[10]There are many listings under References (46-47) not utilized in the work, and presumably they will be used in the generative phonology.

[11]It would have been advisable to publish the texts in the Appendix in narrow phonetic transcriptions (rather than broad) and to have included more texts since the author has made tapes of over 150 university students.

[12]As an illustration Rosén (1962:1) states there is vocalic length in *natắati* 'I planted' but not in *natắti* 'I gave' or (2, fn.2) that *p*, *t*, *k* are never aspirated.

2.3.   Jack Fellman, *The Revival of a Classical Tongue: Eliezer ben Yehuda and the Modern Hebrew Language,*
(Contributions to the Sociology of Language, No. 6, Gen. Ed. Joshua A. Fishman), The Hague: Mouton, 1973.
151 pp.
By *ALAN S. KAYE*   (California State University, Fullerton)

The present volume is a very welcome addition to the literature of a growing field, called
appropriately enough, the historical sociology of language. This is the first work of its
kind to look at the role of one particular individual in the "miraculous" (after Tur-Sinai,
9) revival of Hebrew.   F., rather than seeing the growth of Hebrew in the twentieth century
as any sort of miracle, prefers an analysis that the historical situation and Ben Yehuda
were ripe for a merger which sparked the revival, although he does admit (10) that Ben
Yehuda was not "the reviver of the spoken Hebrew language in any absolute sense," but does
conclude (139) that "for the revival of the Hebrew language in Palestine, that pioneer was
Eliezer Ben Yehuda himself."   The primary focus of the book is to demonstrate that (10)
"Ben Yehuda must be given sole credit and distinction for being the first to state the idea
and the necessity of starting the revival and to be the first to show the feasibility of
implementing it by abandoning his former tongue, Yiddish, and speaking only Hebrew."

The problem of the revival of any language is one which is usually of interest to the writers
of the textbooks since it is apt to enthrall introductory students of linguistic science.
The remarks of Dwight Bolinger (perhaps the best of the texts) are typical of many similar
presentations in the older literature concerning language revival (Hebrew always being the
sine qua non-case) [*Aspects of Language,* 2nd. ed., 1975:576-77]: "In less than thirty years
this small country [Israel] accomplished the most remarkable feat of linguistic engineering
in history." Bolinger goes on to give credit to the schools for teaching "what could almost
as easily have been an artificial language."[1]   And Bolinger also refers to Hebrew as a dead
language which was revived (so also Hockett's textbook [1958:368]). But F., correctly in
my estimation, puts the matter into proper perspective by explaining that (11-12, 113 and
passim) no language has been known to be totally resurrected from the dead. Indeed one of
the major conclusions expressed by F. is that Hebrew was a "half lanugage" until it became
reactivated as a spoken tongue in the 1880's, however, it is, in my opinion, erroneous to
claim that the status of Hebrew beforehand conforms to diglossia in any usual sense of this
term (cf. my paper "More on diglossia in Arabic," in *IJMES* [1975:325-40]) because Jews in
Central and Eastern Europe spoke Yiddish, in Arab countries Arabic, Ladino in Spain, Judaeo-
Persian in Iran, etc. — NOT Hebrew. It is also most doubtful that (13) Hebrew and Latin
were in the Middle Ages the two principal written languages of diglossic Europe.

One of the most interesting things brought out by F. is that Ben Yehuda was a self-taught
Semitist (although he is not referred to as a scholar (129 and *passim*). Ben Yehuda even
went so far as to receive assistance on comparative Semitic problems (lexicography, for the
most part) from Dhorme, Delitzsch, and W.F. Albright.[2] One may conclude that with the
proper linguistic training Ben Yehuda would have made important contributions to comparative
Semitic linguistics, but it should be kept in mind that that was not his purpose in burning
the midnight oil for practical reasons.

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[1]As C. Rabin and others have pointed out, the real credit goes to the immigrants of the many
agricultural sites who wanted to sever all connections with the Galut and the only possible,
even thinkable, cultural milieu for them was Hebrew, and the teachers and schools who
worked so hard to make that desired proclamation a reality (as early as 1903 ALL agricul-
tural settlements had adopted Hebrew for educational purposes (102)).

[2]Albright even wrote about this in two articles about Ben Yehuda published in 1923 (cf. F.'s
bibliography (148)).

It is a great pity that we know so little of Ben Yehuda's own utilization of spoken Hebrew (115-16) except for the facts that his lexical choices were quite different from others (e.g., he used *bɔxavod* for *bɔvakaśa* 'please', *bandūra* for *ʾagvaniya* 'tomato', *ʾamma* 'mother' for *ʾimma*, etc.).[3]

Of great importance is the discussion (83 ff.) as to why the Sephardic pronunciation became koineized. F. presents a thorough list of reasons but underestimates the influence of the local dialects of Arabic and the heritage of literary Arabic as a major factor (cf. M. Kossover, *Arabic Elements in Palestinian Yiddish* [1966], and M. Piamenta, "The Influence of Arabic on the neologisms of Ben Yehuda" [1961]). One can only speculate about whether the Sephardic pronunciation would have won out if a language more closely resembling Ashkenazic pronunciation were a major superstratum in Palestine during the latter part of the nineteenth century.

Since my space is limited I should like to emphasize that F. did a very thorough job, used all the important and relevant materials (even the Hebrew Language Academy Archives) in Hebrew and European languages (cf. the extensive bibliography (140-51)), and made his presentation in a highly organized fashion. Thus I have no hesitation whatsoever in saying that this volume replaces (but not in all respects) the two standard works on this subject, viz., Chomsky's *Hebrew: The Eternal Language* (1957) and Spiegel's *Hebrew Reborn* (1930 and 1957).[4]

3. OMOTIC

3.1   M. Lionel Bender, *Omotic: A New Afroasiatic Language Family* (University Museum Studies, 3). Carbondale: University Museum, Southern Illinois University, 1975. Pp. 4 , viii, 292.

By *PAUL BLACK* (Australian Institute of Aboriginal Studies)

The interrelationships among the major Afroasiatic subgroups have not yet been satisfactorily clarified. The past neglect of this area is well illustrated by the fact that it was only recently that Fleming (1969) recognized that the former Western branch of Cushitic did in fact constitute a distinct "Omotic" family within the Afroasiatic phylum. This hypothesis should hardly be controversial: Moreno (1940) had long ago detailed the substantial differences between Western Cushitic and the remaining Cushitic branches, and more recently Tucker (1967) had also raised doubts about the Cushitic membership of these languages. Subgrouping hypotheses are furthermore not without consequence. The Omotic hypothesis for example implies that those of Dolgopol'skij's (1973) "Proto-Cushitic" reconstructions which attempt to account for "Western Cushitic" evidence should actually be assigned to Proto-Afroasiatic. Perhaps a critical and even-handed reassessment of Afroasiatic subgrouping could lead to yet other significant relevations.

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[3]The reader becomes terribly engrossed as he reads about the various personal problems in the Ben Yehuda household of getting Hebrew used as the chief and only language for everyday communication (114 ff.).

[4]Spiegel's book devotes considerable attention to Ben Yehuda (cf. 390-402 in particular).

Were it not for the past neglect and substantial importance of the higher level subgrouping
of Afroasiatic I would be disinclined to review Bender's present book. It is in fact simply
a research report published in limited numbers[1] in a series intended to "provide early release
to interested colleagues of ... miscellaneous research reports and records" (page prior to
p. i). Whereas the book treats Bender's research on the nature and subgrouping of Afroasiatic
as a whole, its title relates to the only reasonably strong result of this research, namely
its provision of some significant new evidence in support of the Omotic hypothesis. Not sur-
prisingly for such an inconclusive and hastily reported study, the book is furthermore rather
poorly written: lack of a proper introduction obscures the relationship between the title and
the text; the style tends to be wordy, personal, and occasionally repetitious; arguments are
sometimes incomplete and conclusions often understated or overstated; and there are a few
minor inconsistencies, although fortunately few obvious typographical errors.[2] Despite all
this, Bender does bring a fresh approach to an area much in need of further study. His ex-
tensive documentation, including well over a hundred pages of tables listing data and detailing
its analysis, should enable future investigators to emulate the more valuable aspects of his
approach while improving on its deficiencies. Even his less conclusive results furthermore
raise some interesting questions. Could for example at least Semitic and Berber join to form
a single subgroup? Is the traditional view that Cushitic constitutes a genetic unity well
founded? I accordingly decided to review Bender's book, despite its status as a research
report, in the hope of clarifying its usefulness as a basis for further research.

Chapter One of the text is "A Brief History of Afroasiatic Studies" (p. 1-19). This begins
with a rather uncritical listing of the earlier developments and progresses to a somewhat
more critical treatment of a selection of studies in order to "point out both the considerable
accomplishments and the residual problems in the field" (p. 18). Chapter Two (p. 20-37),
"How to Recognize an Afroasiatic Language," continues with a more detailed treatment of the
Afroasiatic (or Hamito-Semitic) characteristics proposed by earlier scholars and ends with
Bender's own list of thirty-six tentative characteristics. Chapter Three (p. 38-48) describes
Bender's "Choice of Representative Languages" for incorporation into the study. Bender sup-
ports his selection by recapitulating Fleming's arguments in support of the Omotic hypothesis
(p. 39-40) and by detailing the internal subgroupings of Cushitic and Omotic[3] and outlining
those of all relevant families (Tables 1 and 2, p. 45-8). The quantitative treatment of
"Afroasiatic Grammatical Characteristics" in Chapter Four (p. 49-123) finds that only fifteen
of the thirty-six tentative characteristics proposed in Chapter Three qualify as "Afroasiatic
isomorphs." The fact that Omotic possesses relatively few of these fifteen is furthermore
taken as evidence that *"Omotic is by far the 'weakest link' in Afroasiatic"* (p. 57-8). The
chapter also discusses Bender's newly proposed characteristic involving restrictions on con-
sonant co-occurrence (p. 59-63), lists the data sources (p. 63-5), and presents some of the

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[1]One hundred fifty copies in the first printing. I am grateful to Bender (personal commun-
ication) for this and other information helpful to me in writing this review.

[2]In the text the name Petráček lacks diacritics (p. 16-7) and "very" occurs where "vary" is
intended (p. 149). In Table 10 at least three abbreviations are misspelled: "MB" should
be "Mb" (item 4, p. 152), one of two occurrences of "Or" should be "Ir" (item 68, p. 183),
and "Hb" should be "Mb" (item 95, p. 196). Cited data could easily contain other errors:
in particular the fact that mubi *hàt* is said to be borrowed from Arabic *baṭn* in the meaning
'belly' (p. 152) suggests a misrepresentation of one of the initial consonants.

[3]Whereas the Cushitic and Omotic classifications are said to be based on Bender (1971),
Werizoid is here reclassified as a branch of Oromoid within Lowland East Cushitic. Black
(1974: Chapter 2) has in fact confirmed Bender's earlier classification of Werizoid as
being at most one of two primary divisions of Lowland East Cushitic.

The two primary divisions of Omotic are labelled "Southern" and "Northern" on p. 46, but
"Eastern" and "Western" respectively on p. 48.

data and its analysis (Tables 3 through 9, p. 66-123). Chapter Five begins a treatment of "Comparative Afroasiatic Lexicon" by illustrating the rather free phonological and semantic matchings characteristic of earlier studies and by stressing the need for stricter controls. He then describes his own lexicostatistical comparisons, at one level on the attested forms and at a second level on his own "instant reconstructions" of proto-forms. Tables 10 through 16 (p. 150-217) present the data and some details of its analysis. The "Summary of Conclusions" in Chapter Six proceeds to draw subgrouping conclusions, graphed in Figure 17 (p. 224), independently on the basis of grammar and lexicon respectively, and concludes by suggesting "a very speculative history of Afroasiatic origins and subsequent movements" (p. 220).

The remainder of the present review deals only with those aspects of Bender's book directly related to his two main avenues of research: his study of Afroasiatic grammatical characteristics and his lexicostatistical comparisons. His methodology is similar for both and is significantly better controlled than that typical of previous studies of Afroasiatic as a whole. Previous studies tended to open-endedly amass whatever data seemed relevant from whatever sources were available, often in order to provide a convincing demonstration of relationship. Bender on the other hand assumes that the relationship has been demonstrated, and accordingly carefully selects limited numbers of languages, grammatical characteristics, and lexicostatistical test items in order to provide a fixed data base for measuring relative degrees of relationship. He is then able to express the relative degrees of relationship in quantitative terms, such as lexicostatistical percentages, which are comparable just because of this fixed data base. When the set of meanings searched for cognates is on the other hand unlimited, the number of cognates actually found in a given language could be dependent on such extraneous factors as how well the language happened to be attested. Bender's use of the standard lexicostatistical constraint of counting only homosemantic cognates furthermore forestalls any question about the semantic appropriateness of his lexical comparisons, thus overcoming a serious criticism typically applicable to earlier studies.

Bender's careful selection of representative languages is a particularly significant factor in improving control. Previous investigators were typically content to draw data primarily from the better attested members of the Afroasiatic families. One striking consequence of this practice was that Omotic data in particular played little role in these studies: not only could Omotic be justifiably ignored because of its former classification as just another branch of Cushitic, but it was also relatively poorly attested and seldom seemed to provide — for good reason, it turns out — much relevant data. Bender on the other hand was careful to select a set of languages representing not only each Afroasiatic family, but also each major branch of each family (p. 42). Semitic is thus represented by Akkadian (of East Semitic), Arabic (Central), and Giiz (South); Berber by Tamazigt and Shilha; Egyptian by a stage of unspecified antiquity; Chadic by Hausa, Mubi, and Margi (presumably representing each of the three branches of Chadic shown in Figure 2, p. 47); Omotic by Dizi (Maji group), Kefa (Kefa group), Welamo (Gimojan group), and Hamer (Eastern Omotic); and Cushitic by Beja (North Cushitic), Awngi (Central), Sidamo (Highland East), Oromo or Galla (Lowland East), and Iraqw (South). Previous investigators furthermore seldom if ever used evidence from non-Afroasiatic languages to demonstrate that (a) their proposed Afroasiatic characteristics were not in fact also characteristic of non-Afroasiatic groups, and that (b) their Afroasiatic lexical comparisons were better and more numerous than those that could conceivably be proposed between Afroasiatic and non-Afroasiatic languages. Bender on the other hand applies his analysis uniformly not only to the above eighteen Afroasiatic languages but also to a control sample of ten non-Afroasiatic languages. The ten control languages include three representing different branches of Nilo-Saharan, Nama and Proto-Central Khoisan representing Khoisan, Proto-Bantu and three attested languages representing different branches of Niger-Kordofanian, and Proto-Indoeuropean.

Whereas Bender's approach was well conceived, its execution leads to largely inconclusive results primarily because Bender's data base simply proved too small to permit meaningful differences to be confidently distinguished from the effects of normal statistical variation.

Bender thus could have done little to improve his analysis in such a way as to significantly improve his results. This is not to say however that the details of Bender's analysis and more especially its presentation could not have been greatly improved.

Bender's study of Afroasiatic grammatical characteristics takes off on the fact that "many scholars have suggested characteristics which one would seek in a language which would suggest that it is likely to be an Afroasiatic language" (p. 20). In his review of the characteristics proposed earlier by Meinhof, Cohen, Vycichl, and Greenberg (p. 21-34), Bender's typical criticism is that some of the proposed characteristics, such as ablaut, are "purely typological and of little relevance for classification" (p. 24). Ablaut and other typological characteristics do however end up on Bender's list of thirty-six tentative characteristics (p. 35-7), where they are presented within a framework that masks their typological nature. Bender's first six characteristics — all of them phonological — are for example placed under a heading claiming that they are features of "A stock of consonant phonemes related by regular sound-correspondence through a proto-language" (p. 34). It would be difficult to apply this heading to the question of whether any single given language possesses the characteristics listed underneath it. In any case however Bender later admits that twelve of his characteristics were included in the tentative list simply because of "their intrinsic interest aside from the genetic question" (p. 53), and that these twelve are to be "shunted aside as too typological to be of much interest for genetic classification" (p. 52-3).

Most of the twenty-four remaining, non-typological characteristics are quite specific about morpheme shape and meaning. Characteristic VIIb for example specifies a "ś ~ š ~ c causative, intensive, or transitive" as a verb derivation marker (p. 37) or, more briefly, simply "ś-causative" (p. 55). Since Bender does not rigidly adhere to the stated meanings (see p. 37) or phonological shapes when positing that a given language possesses a given characterisitc, I suggest that Bender is essentially looking for cognate morphemes. Characteristic VIIb for example could perhaps be read as "possession of a morpheme — of whatever shape or meaning — plausibly cognate to a causative morpheme -ś-." A few of the non-typological characteristics are less morpheme specific; e.g. VIIf: "Vowel change in the first radical" as a verb derivation marker (p. 37).

Table 3 (p. 66-71) summarizes the possession or lack of the thirty-six tentative characterisitcs by the eighteen Afroasiatic and ten non-Afroasiatic languages. Since some of the languages are relatively poorly attested, the table contains many question marks and occasional blanks. Of the twenty-four non-typological characteristics, fifteen are found to be Afroasiatic isomorphs. Bender says that "if a given feature scores + for ten or more of the eighteen representative (Afroasiatic) languages, it is considered to be an isomorph" (p. 53). This turns out to be not quite true. Bender subsequently adds that "we have adopted the procedure of counting a weighted total if a given feature is attested for a given proto-language" (p. 54). If for example a feature is found in two of the four Omotic languages, it is attributed to Proto-Omotic and is counted as if it occurred in all four Omotic languages. Sometimes but not always a feature is attributed to a proto-language if it occurs in only one of the attested descendants. Akkadian for example is the only Semitic language possessing characteristics VIb, c, and d; the last two of these, but not the first, is attributed to Proto-Semitic (see p. 69). Whereas the fifteen Afroasiatic isomorphs score from 10 to 17 out of a possible 18 on this basis (p. 55), characteristic VId is in fact possessed by only seven of the attested Afroasiatic languages, characteristic Vc by only nine, and the remaining isomorphs by from ten to fifteen. As Bender (p. 55) notes, none of the fifteen Afroasiatic isomorphs is possessed by even a simple majority of the non-Afroasiatic languages.

It seems appropriate for Bender to weigh his evaluation of the characteristics in terms of their distribution in Afroasiatic families. If this were not done in some way, a characteristic possessed by, say, only twelve Cushitic languages could superficially appear to be much better than one possessed by one or two languages of several of the remaining five Afroasiatic families. Bender's weighing of the "attestation" of a feature in a proto-language

according to the number of represented descendants does not however avoid such a potential problem: the weight can be changed as desired simply by altering the number of representative languages. Bender accordingly does not provide a convincing basis for evaluating Afroasiatic characteristics quantitatively. He does however provide much more explicit statements about the distribution of the characteristics throughout Afroasiatic than generally found in earlier studies.

Bender goes beyond what initially appears to be an intrinsic interest in the characteristics by ultimately taking them as a basis for subgrouping. On the most basic level he notes that Proto-Omotic possesses only four of the fifteen Afroasiatic isomorphs whereas the remaining Afroasiatic proto-languages possess from eleven to fifteen each. The ranges of the number of isomorphs possessed by the respective attested descendants are similar. Bender accordingly claims that Omotic is the "weakest link" in Afroasiatic (p. 58) and in fact "the oldest branching in the Afroasiatic family tree" (p. 218). Whether or not this is entirely justified on the basis of Bender's grammatical results alone, it does significantly agree with his subsequent lexicostatistical results. Bender later presents, but neither justified nor presses, a more detailed Afroasiatic family tree based on grammar (p. 224). Omotic aside, a difference of one or two characteristics more or less would hardly seem to justify differences in subgrouping.

Bender's first level of lexicostatistical comparison is based on the attested forms for ninety-eight meanings in his representative languages plus Proto-Chad; Table 10 (p. 150-98) lists the forms and decisions on cognation. A check of the forms and cognation decisions for Oromo (or Galla), the representative language I am best acquainted with, suggests that they are generally reasonable. The phonological transcription is sometimes imprecise, but the lexicostatistical consequences are negligible. I would change one Oromo form — Bender's *ɭaɭ-* (perhaps *iɭaaɭ-* 'look at') to *aʌg-* for 'see' — and quibble with perhaps as many as a fifth of his cognation decisions. Even so my own recomputed Oromo percentages almost never differ from Bender's by more than one or two percentage points. The greatest difference is in the percentage shared by Oromo and Iraqw, which would be lowered from Bender's 15% to my 11%; this however serves only to make this percentage more in accord with the percentages of Iraqw with the remaining Cushitic languages. Whereas Bender's percentages could thus be refined, I doubt that refinements would change them substantially.

Table 12 (p. 202-4) lists Bender's lexicostatistical percentages among the representative languages. Bender's further analysis is largely confined to listing, for each Afroasiatic and non-Afroasiatic family, the range of percentages between different groups (Table 15, p. 216) and a variety of diagnostic values for each group (Table 16, p. 217). The diagnostic values are the "internal high," the "internal low," the "internal average," and the "external high," as previously defined by Bender (1971). These values are then taken as a basis for judging how well supported the various groups are: Cushitic for example is problematic because its external high is generally higher than its internal low and occasionally even higher than its internal average (p. 144). Whereas Bender touches on a number of such problems, he does not provide a complete analysis of the percentages — only at one point (p. 220) does he touch on subgrouping within a single family — and he does not provide an explicit justification for his ultimate family tree based on lexicon (p. 224).

Bender could have presented a more solid analysis of the percentages; I accordingly offer my own analysis below. I will however ignore Bender's inclusion of Proto-Chad, since Bender correctly observes that Proto-Chad has a relatively high and wide range of percentages largely because there were less than sixty Proto-Chad forms available for comparison, and perhaps also because of "convergence through reconstruction" (p. 146). I also hold the percentages of the non-contemporary Akkadian, Giiz, and Egyptian to be largely incomparable with those of the contemporary languages because the time periods in which lexical divergence could take place are unequal, a fact not noted by Bender. I also take into account the fact that two percentages based on a test list of about a hundred items are best treated as significantly different statistically only if they differ by about 14 percentage points or more in general

or, considering that many of Bender's percentages are below 20%, perhaps a few percentage points less than this in the specific case at hand (see Black forthcoming: fn. 3).

My analysis of the percentages in Bender's Table 12 (p. 202-4) proceeds as follows. Shilha and Tamazigt first clearly group to form Berber at 57%. Arabic can furthermore probably be safely considered to group with Akkadian and Giiz to form Semitic at 42% to 50%. Because of the non-contemporary nature of the latter pair of languages the percentages of Arabic alone are best taken to represent those of Semitic in further analysis. Three other previously proposed groupings are supported rather weakly by joining percentages only eight to ten percentage points higher than the highest outside percentage. These include the grouping of Kefa and Welamo to form Kefa-Gimojan at 29%, the grouping of the preceding pair with Dizi and Hamer to form Omotic at 14% to 19%, and the grouping of Sidamo and Oromo to form East Cushitic at 23%.

There is a similarly weak basis for recognizing two subgroups not to my knowledge seriously proposed elsewhere. One is a grouping of Semitic and Berber on the basis of Arabic's 31% with Shilha and 20% with Tamazight. Bender ultimately (p. 224) shows such a grouping in a family tree, but otherwise makes little of it. He regards the 31% between Arabic and Shilha as a "contact-situation resultant" (p. 145) and suggests further that Shilha tends to score higher than Tamazight with all the remaining languages because of "greater historical contact with the outside world in the case of Shilha" (p. 146). Whereas this could perhaps be true, an examination of Bender's cognation decisions suggests that Bender has systematically excluded any obvious borrowings. It furthermore reveals that only 73 of the 98 test items are attested for Shilha: a full quarter of the test list is missing. Bender does not mention this significant fact which could perhaps account for Shilha having somewhat higher percentages than Tamazigt. Even ignoring the Shilha percentages, the 20% (19% by my recount) between Arabic and Tamazigt is high enough to weakly justify speculation about a possibly subgrouping of Semitic with Berber. The highest outside percentage of 16% is with a language, Mubi, which lacks 14 of the 98 test items; this percentage is furthermore lowered to 13% by my reappraisal of cognates.

Whereas the non-contemporary nature of Egyptian makes its percentages incomparable with those of the contemporary languages, it does score noticeably higher with Semitic and Berber — from 13% to 17% — than with the remaining Afroasiatic languages — from 3% to 8%. Egyptian could thus conceivably join a subgroup containing Semitic and Berber. Bender does show such a subgrouping in his family tree (p. 224) and elsewhere speaks of Semitic, Berber, and Egyptian as lexically forming part of an "orthodox core" (p. 218).

A second weak and apparently previously unproposed grouping is that of Hausa and Mubi. This pair shares 23% with each other as against their percentages of 13% to 15% with Margi, the third Chadic language. The latter range of percentages is clearly not significantly higher than the percentages between Chadic on the one hand and Semitic and Berber on the other. Ignoring the Mubi percentages discussed earlier, the percentages between Chadic and either Semitic or Berber average over 11% and range as high as 14%. Chadic itself is accordingly not even weakly supported as a subgroup.

Cushitic also finds extremely poor support as a subgroup. Ignoring the 23% between Sidamo and Oromo taken earlier to support an East Cushitic subgroup, most of the percentages within Cushitic range from 11% to 13%, one being as low as 7% and two as high as 15%. The percentages between Cushitic on the one hand and Semitic, Berber, and Chadic on the other range largely between 6% and 11%, on being as low as 4% and one as high as 13%. The difference between a 11% to 13% range and a 6% to 11% range could perhaps be meaningful, but is clearly far from statistically significant. Bender discusses the Cushitic problem at some length (p. 219-20), noting that it could perhaps be appropriate to stop treating Cushitic as a genetic unity. This is only to say that the genetic unity of Cushitic has not yet been

conclusively demonstrated. In particular it cannot be demonstrated on the basis of a 98 item lexicostatistical list because any meaningful difference between the internal and external percentages of Cushitic tends to be swamped by the effects of normal statistical variation. The low percentages within Cushitic, indicative of far more lexical divergence than found in such families as Semitic and Berber, are incidentally quite in accord with percentages calculated earlier by Bender (1971) and Black (1974:29).

The percentages between the Omotic languages and the remaining contemporary Afroasiatic languages range from 1% to 11% and average less than 5%. This range is only slightly lower than the 4% to 13% range of percentages between other pairs of Afroasiatic families and at the same time only slightly higher than the 0% to 8% range between Afroasiatic and the non-Afroasiatic control languages. Such small differences do not merit a precise subgrouping of Omotic, whether within Afroasiatic or not. The percentages of Omotic with Cushitic — ranging from 2% to 11% and averaging about 6% — do on the other hand tend to support the exclusion of Omotic from Cushitic. These percentages are first of all not markedly higher than the percentages between the Omotic languages and other Afroasiatic languages — these range from 1% to 9% to average 5%. Ther percentages of Omotic with Cushitic are furthermore noticeably lower than the percentages within Cushitic, all but one of which are 10% or more. Bender not only fails to note this, but also ultimately — and quite oddly in view of the title of the book — subgroups Cushitic and Omotic together in his family tree based on lexicon (p. 224), and in his conclusion speaks of a "Proto-'Cushomotic'" proto-language (p. 222).

Bender's second level of lexicostatistical comparison was applied to the proto-languages of each of the Afroasiatic families and non-Afroasiatic groups. Bender first set up proto-forms for each family "by inspection" in each instance where half or more of the languages representing the family share cognate forms (p. 139). These "instant reconstructions" (Bender's term) seem of little intrinsic interest: Awngi *iŝŝan*, Sidamo *ŝano*, and Iraqw *duŋgaʔ* 'nose' (p. 179) are for example taken as a basis for proposing a Proto-Cushitic form spelled *ŝ,d-a,u-n-* (p. 210). Bender is furthermore generally unable to provide proto-forms for even a majority of items on the test list: only twenty-one are for example proposed for Proto-Cushitic. The results are well summarized by Bender (p. 149): "...the sample of comparisons possible is generally too small to be significant... This leaves little to be said about the results ... ...the sample sizes very [sic] too much to make detailed analysis worthwhile." Bender could perhaps have done better if he had not worried about proto-forms but rather had attempted to reconstruct only the decisions of cognation for the proto-languages. If for example one Semitic and one Chadic language truly share a cognate in a given meaning, it would follow that Proto-Semitic and Proto-Chadic must also have shared a cognate in this meaning.[4]

Bender ultimately uses his grammatical and lexical results as a basis for proposing that the Afroasiatic languages spread out from an original homeland located in the region of Ethiopia or the Sudan (p. 220-3). The evidence in support of Ethiopia as an original homeland seems firm: not only does this region contain languages belonging to three distinct Afroasiatic families — Semitic, Omotic, and Cushitic — but the last of these three is furthermore characterized by such a high degree of internal divergence that its status as a genetic unity can be questioned. Bender only spoils his argument by returning, without any justification whatsoever, to the view that Cushitic and Omotic shared a common ancestor, "Proto-'Cushomotic'" (p. 222), subsequent to Proto-Afroasiatic.

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[4]This statement is intended only to demonstrate the possibility of reconstructing cognate decisions without reconstructing proto-forms. There is at least one other reasonable way of assigning cognates to a proto-language and two ways of assigning non-cognates. The approach is thus not altogether straightforward and apparently has not yet been applied. It is not however a new idea; I first heard of it from Prof. Isidore Dyen of Yale University.

In summary, Bender's book must be read with caution: it tends to be more valuable for what
Bender does than for what he claims. Its specific contributions to our knowledge of
Afroasiatic are few. Its evidence in support of the Omotic hypothesis is significant because
it leaves the sceptic with little grounds for argument, whether on the basis of grammar or
lexicon. Beyond this it has made the study of Afroasiatic grammatical characteristics some-
what more explicit and has pinpointed problems that have yet to be treated decisively, such
as the genetic basis of the Cushitic grouping and the relationship between at least Semitic
and Berber. The book contributes more by way of methodology. Bender's carefully controlled
quantitative approach provides a much more objective basis for evaluating evidence than has
previously been used in the study of Afroasiatic. Future investigators should be able to
improve on his imperfect application of this approach and follow it to its logical consequences.
Refining the data base and increasing its size would especially contribute to the conclusive-
ness of the results. Bender's extensive documentation makes the pursuit of such improvements
much easier than it might otherwise be.

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- *Cuneiform Texts of the 3rd Millennium in Old-Canaanite from the 1974 Season at Tell Mardikh = Ebla.* By G. Pettinato.

Volume 2

- *Tales of the Old Gods and Other Essays.* By T. Jacobsen.

AFROASIATIC DIALECTS

Afroasiatic Dialects (*AAD*) seeks to provide concise descriptions of individual languages which belong to the Afroasiatic language family. It is primarily directed toward an audience consisting, on the one hand, of students of one or several Afroasiatic languages, and, on the other, of students of linguistics. In these volumes, both these groups should find succinct treatises such as to provide familiarity with the basic structure of the language in question in a comparative perspective. Each description will be comprehensive in scope and sufficiently detailed in exemplification. But at the same time the aim will be to cut through to the essential and to avoid specialized argumentation. The goal then is neither to publish a corpus of exhaustive reference grammars nor to provide a platform for the analytical defense of theoretical questions. In this sense the series is properly data-oriented. Though the authors will necessarily be of a variety of theoretical persuasions and each will have his own set of preferences for presentation, not the least important goal will be to achieve as high a degree as possible of uniformity in structure, and in the conventional signs and terminology used. This being accomplished, the reader should have no difficulty in finding points of resemblance and divergence amongst the languages which concern him with regard to some point of inquiry. The term 'dialects' in the series' title refers not only to modern spoken vernaculars but to historically definable stages of any language of the various branches. Publication of studies of as many such dialects as possible would provide, we feel, both an encouragement to comparative work and a sound documentary base on which alone this work may fruitfully progress.

AAD 1 - Berber: *TAMAZIGHT OF THE AYT NDHIR* by Thomas G. Penchoen. 1973, IV-124 pp., $8.50.

The Ayt Ndhir dialect which is described belongs to one of the major Berber languages, Tamazight, spoken in the Middle Atlas Mountains of central Morocco. The description is based in the main on research undertaken with native speakers of the Ayt Ndhir territory surrounding El Hajeb. — While directed to the non-specialist, a number of points in the description proper will be of interest to the specialist as well: the presentation of noun and verb morphology points up a number of regularities which more often than not have been obscured in previous descriptions. Also, phonological rules are given which account for the major share of morphophonemic complexities. The reader will find in the appendices and 'optional' sections conjugation tables of typical verbs—including detailed observations on the placement of shwa in verbs—, a chart showing the main morphological patterns involved in verb derivation, a description of the phonological rules applying in complex sequences of morphemes of the verb group, the 'basic' vocabulary contained in several well-known lexicostatistic word lists, and a chart of the Tifinaɣ alphabet used by the Tuareg.

AAD 2 - Ancient Egyptian: *MIDDLE EGYPTIAN* by John Callender. 1975, 150 pp., $10.

This grammar deals with the literary language used in Egypt from ca. 2000 to 1200 B.C. and considered in even later times to be the classical written form of Egyptian. The book is directed toward the general linguist as well as the Egyptologist; examples are glossed and written in transcription and there is an index of grammatical terms and Egyptian morphemes. A comprehensive set of paradigms of both verbal and non-verbal predicate types is included as an appendix, together with an appendix on negation and one on the historical origin of certain constructions. — The grammar contains three main parts: phonology, morphology, and syntax, of which the last receives most emphasis. The section on phonology sketches the laws of sound change to the extent they can be discovered. The section on morphology stresses the paradigmatic character of verb tenses and their derivations. A distinction is made between truly paradigmatic tenses and tenses borrowed from Old Egyptian for quotations or special effect. Following Polotsky, the "emphatic forms" are treated as nominalizations under the rubric "manner nominalizations." Unlike previous grammars of Egyptian, this grammar discusses syntax according to transformational categories. The process of "clefting" interrelates emphatic forms, the "participial statement" and constructions with *pw*+ relatives. The process character of negation is emphasized, and the implications of so considering it are developed in a special appendix. A sample text is also included, accompanied by a vocabulary and a translation.

AAD 3 - Semitic: *DAMASCUS ARABIC* by Arne Ambros. In preparation.

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